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Long-run consequences of natural disasters: Evidence from Tangshan

By Guo Xu[1]

Abstract

Exploiting Tangshan 1976 - the deadliest earthquake in the 20th century - as a source of exogenous variation, we estimate the long-run effect of a historical shock on contemporary socio-economic outcomes. Cohorts born after the earthquake were not only larger, but exhibit lower school completion rates, particularly among the female today. Despite lower schooling levels, there is no evidence for adverse labour market outcomes. We conduct robustness checks and argue that the effect is causal.
Keywords: Environmental shock, earthquake, natural disaster, education, fertility
JEL codes: I20, J00 and O18

1 Introduction

This paper examines the demographic consequences of large-scale environmental shocks. In contrast to existing empirical contributions that focused mainly on short-run impacts, we identify long-term effects by exploiting Tangshan 1976 - the deadliest earthquake in the 20th century (EMDAT, 2011) - as a source of exogenous variation. By building a pseudo-panel based on county-level 2000 census data, we then isolate the effect using a standard difference-in-differences (DiD) strategy.

We provide confirmatory evidence for a positive fertility response after the earthquake: Cohorts born in 1976 - 1978 are significantly larger in counties subject to more severe levels of destruction. These cohorts today exhibit lower rates of schooling, particularly among the female: Counties and cohorts exposed to the strongest shocks 25 years ago exhibit secondary completion rates that are about 25% lower on average, with the sex ratio almost doubled. Finally, there is no evidence that the earthquake had negative effects on labour market outcomes: Even though educational levels were lower, the quake did not exert a significant effect on unemployment rates.

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These results are robust and causal: We provide systematic evidence for the exogenous nature of the shock and conduct a wide range of robustness checks to rule out econometric concerns, including placebo tests, test of common trend assumption, accounting for migration and the inclusion of alternative measures for assignment to treatment group. Despite the pseudo-panel, the results remain stable. Taken at face value, these results suggest that the effect of large one-off shocks are much more persistent. If so, disaster relief and related post-shock interventions should depart from a short-run perspective and focus on cohort-specific measures to cushion large and unexpected shocks.

The remaining paper is organized as follows: Section I offers a brief literature review on the demographic impacts of natural disasters, identifying gaps and laying the theoretical framework for this paper. Section II introduces the context of Tangshan 1976 and the empirical strategy employed to identify the long-term effect. Section III presents and discusses the results. Section IV concludes.

1.1 Impacts of natural disasters

The impacts of natural disasters can be decomposed in risk and shock (Baez et al., 2010): On the one hand, natural disasters can increase the baseline risk of the environment, thereby permanently shifting the preferences and behaviour of individuals. In theory, such risk increases the discount rate, thereby rendering activities that generate long-term benefits at short-term costs, such as saving or education, less attractive (Lorentzen et al., 2008). In addition, higher mortality can also exert an "insurance effect" (Rosenzweig and Stark, 1997). In absence of credit markets, children often enable consumption smoothing, where the costs of child-rearing are later compensated by additional labour and income transfers (Foster, 1995; Townsend, 1994).

On the other hand, shocks may also induce behavioural changes *ex post*. As Finlay (2009) argues, large shocks can generate "replacement effects", where additional children are born in response to compensate for the children lost. Large shocks not only coincide with destruction of human capital but also physical capital, thereby possibly pushing vulnerable households into poverty traps (Carter et al., 2007). This shift in capital-labour ratio can change intra-household allocation, thus diminishing returns to education in face of higher marginal productivity of labour and capital (Jensen, 2000).

Two closely related papers are worthwhile discussing in greater detail: Baez and Santos (2007) provide quasi-experimental evidence from Nicaragua. By exploiting the exogenous trajectory of Hurricane Mitch for assignment to treatment group, they estimate the short-run effect of large weather shocks on child well-being. Combining repeated cross-sectional LSMS data with data obtained from the Demographic Health Survey (DHS), they employ a DiD strategy to isolate the weather effect. Their results suggest that children in affected areas were more likely to be undernourished and taken for medical consultation. Even though the labour force participation rates increased among the children, there was no significant effect on school completion.

While Baez and Santos (2007) offer detailed evidence for Nicaragua, natural disasters are heterogeneous and not likely to yield the same overall effect (Cavallo et al., 2010). The work closest to ours is that of Finlay (2009), who examines the fertility response to three earthquakes in Turkey 1999, India 2001 and Pakistan 2005 based on repeated cross-sectional data from the Demographic Health Survey (DHS). Likewise employing a DiD strategy, she provides evidence for a positive fertility response.

Our findings are in line with Finlay (2009) but differ in several important ways: First, we provide unique evidence from an earthquake in China, a country most affected by natural disasters but in this context only little studied (Cavallo and Noy, 2009). Second, while both papers focus on the immediate effects, we adopt a long-run perspective and estimate the effect of the earthquake quarter a century later, thereby adding value to the literature that explores how extreme conditions in early childhood shape outcomes in later life (Dreze and Sen, 1989; Barker, 1998). Finally, we demonstrate that reliable findings can be estimated based on a single census. Since panels or repeated cross-sections are often unavailable in developing countries, our method is thus applicable to a much greater set of countries.

2 Empirical strategy

2.1 Tangshan 1976

Tangshan, located in the province Hebei, is part of the North China Plain bounded by a mountain range (Yanshan) on the north and by the sea (Bohai) on the southwest. Two major national central cities, the capital Beijing and the port Tianjin, lie in its close vincinity. Several morphological features are evident in the Tangshan region: "To the north, close to the Yanshan mountain range, is a hilly region of elevation less than 500 m above sea level. At the bottom of the hills, erosion processes resulted in fan-shaped alluvial plains. The geomorphology has been modified by the flow of Luanhe and several other major rivers which run through the Yanshan mountains and the plain to reach Bohai. The coastal region is relatively flat at about 5 m above sea level" (Yong, 1988). This morphological variation is key to our empirical strategy as it generates the crucial variation in earthquake intensity needed to elicit long-run effects.

Officially awarded city status in 1938, Tangshan is a relatively young city. Over the decades and particularly after World War II (WWII), it has grown into an industrial center following developments in the coal, cement and ceramics industry. Tangshan has also been an important junction of the national transportation system as well as the telecommunication network. At the onset of the earthquake, the total population of city and municipalities had reached about 1 million and 6 million, respectively. Tangshan has also been an important agricultural area, with main agricultural products being wheat, corn, rice, sorghum and soybeans (Yong, 1988). In socio-economic terms, Tangshan is often considered to be an "average" Chinese coastal region (Huixian, 2002).

On 28 July 1976 at 3:42 am, an earthquake of magnitude 7.8 shook Tangshan. The earthquake was felt within an area of of 2,167,000 km^2 covering Beijing, Tianjin and 12 neighboring provinces and the autonomous regions. With the epicentre close to the city, the city centre was essentially reduced to rubble. Overall, the earthquake is estimated to have killed 240.000 people (EMDAT, 2011). In terms of fatalities, this was the deadliest earthquake recorded in the 20th century. Figure 1 and 2 present two historical photographs to illustrate the extent of the overall destruction. Reconstruction began in 1978, with significant assistance by the 14 neighboring provinces and cities (Gere and

Shah, 1980). Today, Tangshan's city population again exceeds 1.3 million.

Despite the tragedy and immense suffering the earthquake has brought, Tangshan 1976 is an ideal natural experiment to identify the effect of a large-scale shock: First, the shock was large enough to have had substantial impact on a region with fairly balanced socio-economic indicators. Second, the specific morphological characteristics have generated additional variation to the shock, rendering it even more exogenous. Finally, migration has been fairly limited due to the rigid household registration system (*hukou*), mitigating biases driven by selective inflow or outflow of population. If large and unexpected disasters have any long-run effects, they must be found in Tangshan.

2.2 Identification

In order to identify the effect of the earthquake, we exploit differential earthquake intensity as a source of exogenous variation. The earthquake is exogenous in terms of location and time: With regard to location, the exact epicentre of the main shock[2] is determined by geographical characteristics. As such, the spatial variation in shock intensity is not causally related to socio-economic variables of interest. With regard to time, the exact timing of the main shock (3:42 am on 28th July 1976) depends on seismic dynamics and stochasticity that are independent of time-variant socio-economic variables.

But since the frequency of quakes is correlated with geophysical features, it is possible that households and individuals have included earthquake risk as part of their expectation. Poertner (2008), for example, constructs a measure of baseline risk for Guatemala by drawing upon hurricane data of the past 120 years, finding a significant effect of historical risk on current household behaviour. For earthquakes, regions lying near faults or tectonic boundaries are often subject to regular minor shocks. California, for example, is located on the San Andreas Fault, which forms the tectonic boundary between the Pacific and North American plate. Given the recurrence of quakes, institutional arrangements were set up in response, ranging from earthquake prediction, earthquake-proof housing and construction standards to disaster relief infrastructure and emergency training. But if earthquake risk is indeed correlated with both geographic characteristics and socio-economic indicators, the quake itself will not be exogenous, hence prohibiting

[2]Located at $39°38'N, 118°11'E$ (Huixian, 2002)

identification using differential earthquake intensity.

The Tangshan quake, however, is truly exogenous: In contrast to interplate quakes often occuring on plate boundaries, Tangshan was subject to a rare intraplate earthquake (Stein and Mazzotti, 2007). The exceptional magnitude of 7.8 does not fit into the historical seismicity pattern. According to Yong (1988), the strongest quake occuring in immediate vicinity of Tangshan was "only" 4.75. Given the historically low risk of earthquakes, Tangshan was classified a low earthquake zone, with little consideration given to earthquake resistant building codes. As Lomitz and Castanos (2007) write:

> "The Tangshan fault was well known to geologists, but it had been believed to be inactive since the Oligocene times [...] No one foresaw a need to provide increased earthquake protection at the intraplate location."

Coupled with the lack of foreshocks that would have enabled preventive measures, all existing evidence suggests that the Tangshan quake came unexpected and exogenous.

Figure 3 illustrates the spatial distribution of the shock intensity across counties. The measure for intensity is recorded maximum ground acceleration taken from (Huixian, 1986), with intensity VI as the cut-off. Two points are worthwhile re-affirming: First, the magnitude of the shock was large: The 400 km^2 subject to the strongest levels of intensity X-XI saw nearly 100% destruction of buildings and infrastructure. Counties with intensity IX suffered about 40% destruction, covering about 1800 km^2. Intensity VIII and VII are associated with about 10-30% destruction, with an area affected as large as 40,000 km^2. Second, the shock intensity exhibited a great variation: For example, the Liaohe region along the Bohai faced anomalously high intensities (IX), while the neighboring Yutian county survived the quake with low intensity (VI). These differences are driven by exogenous morphological features: Liaohe had a relatively thick sedimentary layer on the top of which were accreted alluvium, favourable conditions to transmit shocks. Yutian, on the other hand, is located in an alluvial fan, with a layer of clay, coarse sand and pebbles with no active faults evident (Yong, 1988). Combined, both features enable a rigorous estimation of the earthquake's treatment effect.

2.3 Empirical model

The main identification strategy is operationalized using a standard difference-in-differences (DiD) estimation. The empirical model is formulated as:

$$y_{it} = \beta_0 + \beta_1 \times post_t + \beta_2 \times intense_{it} + \beta_3 \times post_t \times intense_{it} + \Sigma_{k=4}\beta_k \times z_{kit} + \epsilon_{it} \quad (1)$$

where y_{it} denotes the outcome for county i at time t. $post_t$ is an indicator variable assuming the value 1 from 1976 to 1978. The variable is lagged for 2 years to capture both the immediate and intermediate effects. $intense_{it}$ is a measure for the intensity of the earthquake. Unlike existing papers where treatment is binary, we adopt a more differentiated continuous measure. The interaction between $post_t$ and $intense_{it}$, the main effect, picks up the one-off causal effect of the earthquake. Finally, z_{it} denotes a battery of controls. Despite the exogenous shock, both groups are less balanced than it would be in truly randomized controlled trials. By controlling for observable confounds, we hope to isolate most of the differences between treatment and control group. Finally, e_{it} is the disturbance term, capturing measurement error and residual confounds.

2.4 Data

While comparing pre-quake and post-quake variables across treatment and control group is a reliable identification strategy, the lack of panel data complicates a straightforward application. To address this issue, we derive a synthetic panel based on a single cross-section by exploiting the implicit longitudinal dimension of census data: Essentially, we compare cohorts born in 1976-1978 ($post_t = 1$) to cohorts born 1969-1975 and 1979-1985 ($post_t = 0$). A person aged 23 in the 2000 Chinese Population Census, for example, is born 1977, one year after the earthquake. By comparing cohorts exposed to the shock to cohorts unaffected and interacting the timing with the spatial variation in intensity, it is possible to construct a "pseudo-panel" that allows for a DiD estimation.[3]

As a crucial assumption, the observed age-cohort size in year 2000 needs to adequately approximate the actual birth-cohort size. The main concern here is that the age-cohort is an underestimate of the birth-cohort size, particularly if mortality is high. But even

[3] A similar strategy has been employed by Meng et al. (2010) who study the Chinese Great Famine and approximate famine intensity using age-cohort sizes. More recently, Li et al. (2010) have also adopted this strategy for studying the one-child policy based on 1990 census data.

if this could pose a severe problem for older ages, it is likely to be of little concern for the age groups sampled (age 15-31 years). By the end of the 1970s, Chinese mortality had already declined significantly (Dreze and Sen, 2002), with life expectancy as high as 65 years in 1975-1980 (UNPD, 2008). A more serious issue is a systematic bias induced by the one-off increase in mortality caused by the earthquake fatalities: For example, it is possible that the effect of the earthquake could be upward biased since the pre-1976 age-cohorts are smaller due to the earthquake. While this case cannot be ruled out, it can be controlled for, particularly if the direction of the bias is known.

The 2000 Population Census provides county-level data on a variety of socio-economic variables. Along our research question, we chose age-cohort size, the secondary ("middle school") completion rate by age and unemployment by age as the main outcome variables. Despite the exogenous nature of the shock, we control for a variety of possible confounds to increase the precision of the estimates: The main concern here is the closeness of the epicentre to the urban centre of Tangshan. If counties most severely hit by the earthquake are located in urban regions, we might partly pick up urban/rural differences instead of the direct earthquake impact. Fortunately, most of these differences are time-invariant, so we can include county area and a rural/urban dummy to capture them. In addition, we also include three types of time-varying controls: First, we control for aggregate demographic volatility using the average provincial crude birth rate (CBR) and crude death rate (CDR)[4]. Second, we use annual fixed effects to capture year specific drivers all counties are equally exposed to. Finally, we add a linear and quadratic time trend to account for differences driven by long-run movements such as the demographic transition. Overall, this leaves a final sample of 65 counties, with 45 affected administrative units[5] in Hebei, 18 units in Tianjin and 2 units in Beijing.

We use two measures for shock intensity: The first measure is ordinal and based on estimates by Huixian (1986) and Mei (1982) who measure the spatial distribution of earthquake intensity using recorded maximum ground acceleration. The recorded levels of intensity range from magnitude VI to XI. The second measure is cruder but continuous, where earthquake intensity is proxied by the distance (km) of the county from the earthquake epicentre. As Shiono (1995) illustrates, the distance from epicentre is a

[4]The data comes from the compendium CSP (1999), "Comprehensive Statistical Data and Materials on 50 Years of New China".

[5]The administrative units are on the same level, county or district.

relatively precise way to estimate collapse and fatality rate functions. Both measures are negatively correlated, with a linear correlation coefficient of -0.7. The distance is calculated as the minimum distance (single-linkage).

One often neglected problem of DiD estimates is the high degree of positive serial-correlation: This issue is well illustrated in the Monte Carlo simulations run by Bertrand et al. (2002), where ignoring the serial-correlation leads to overbloated t-statistics and large Type I errors. Unlike Finlay (2009) who does not even mention this problem, we address it in two ways: First, we employ a radical solution proposed by Bertrand et al. (2002) and ignore the time dimension by averaging pre and post data, thus generating a panel of length 2. This extreme simplification is possible in this exceptional setting since the earthquake affects all counties at the same time. Second, we cluster at the county level, a specification that generates consistent estimates as long as the number of counties is large enough (Kezdi, 2005).

3 Results

3.1 Descriptive statistics

Table 1 tabulates the pre-shock summary by treatment status. By definition, a county is "treated" if it has been exposed to an earthquake intensity of X-XI. The simple tabulation of means suggests that most variables are unbalanced across treatment and control group: Cohort sizes in treatment groups are significantly smaller, exhibit lower sex ratios, higher secondary completion rates and unemployment rates. Most of these unbalanced characteristics, however, are driven by the proximity of the epicentre to the urban areas of Tangshan: Not only are administrative units in urban regions significantly smaller - urban regions also tend to exhibit smaller cohort sizes, higher secondary completion and unemployment rates. While the treatment and control groups are less balanced than in truly experimental settings, the results of next subsection suggest that the imperfect randomization would have at best downward biased our results.

3.2 Estimation

We proceed as follows: First, we employ the DiD strategy to estimate the effect of the earthquake on subsequent cohort size (Table 2). We then apply the same strategy to estimate the effect on various secondary outcomes (Table 3).

Table 2 reports a detailed regression for cohort size. Along the assumption that quake intensity *intensity* is exogenous, the magnitude of the earthquake in 1976 had no significant effect on the average cohort size between 1965 to 1985. Without interaction effects, the benchmark specification would suggest that the cohorts coinciding and following the earthquake (*quake_lag*) are significantly smaller (Column I). Once including the interaction *intensity* $\times$ *quake_lag*, however, the sign switches.

The main effect is statistically significant, positive and large (Column II): Cohort sizes in counties subject to a large shock in 1976 saw significantly larger increases in 1976 - 1978. While a county subject to low earthquake intensity VI would exhibit a decrease in cohort size by about -17.7% on average, a county hit by the intensity XI would see a rise by 16.3%. These estimates are robust upon the inclusion of additional controls: Including county fixed effects to capture unbalanced urban-rural differentials even increases the coefficient size of the main effect (Column III). We also add aggregate CBR and CDR (Column IV), a quadratic cohort trend (Column V) and time fixed effects (Column VI) to address time-variant confounds, but the results remain remarkably stable.

We repeat this exercise for the various secondary outcome variables (Table 3), but now only reporting the full specification: Column I uses the cohort sex ratio as the dependent variable. In contrast, the main effect is now insignificant, suggesting that the earthquake had no effect on the sex composition of the affected cohort. The estimates do suggest, however, that the earthquake had adverse effects on the educational outcomes: Cohorts subject to stronger shocks experienced lower secondary completion ratios (Column II). Counties most severely hit by the earthquake, for example, would see an average ratio -25.3% lower. In addition, the quake appears to induce a shift in the sex composition of enrolled students. The sex ratio of enrolled students is now significantly shifted towards male students (Column III). On average, the quake is associated with an increase in school completion sex ratio as high as 96%. Finally, there appears to be no substantial effect on unemployment rates: Cohorts exposed to the earthquake do not exhibit significantly different unemployment rates (Column VI). Even though there is some evidence for a change in sex composition among the unemployed and affected cohorts, the effect is not highly significant and more ambiguous (Column V).

3.3 Robustness checks

We conduct a variety of robustness checks to support our results. First, the findings are not driven by our measure for earthquake intensity. Table 4 repeats the regression using distance to epicentre (km) as an alternative measure: Since intensity levels decrease with distance from epicentre, the interaction term is now significantly negative but this does not substantially change the previous results apart from interpretation.

Second, a more grave concern is that the observed increase in cohort size does not reflect a fertility response but instead captures the fatalities. Since the earthquake diminished the population born before 1976, it is possible that the increase is an artifact of the discontinuity. If so, this effect should be captured by a shift in the intercept and not affect the slope, as captured by the main effect: To test for an intercept shift, we include $before$ - a dummy valued 1 before the earthquake and 0 after - into our full specification (Table 5, Column II). While there is indeed a "jump", controlling for this shift even increases the magnitude of the main effect: As expected, $before$ is highly significant and negative, capturing the adverse impact of the earthquake on the pre-1976 population. Accounting for this, the main effect now increases by almost 0.5% points in magnitude.

Third, we conduct several placebo tests to show that the effect is indeed attributable to the earthquake (Table 5): Column III and Column IV report results based on placebo earthquakes in 1973 and 1970. In both regressions, the main effect is insignificant. Combined with the DiD strategy, it is hard to argue that there could have been any other effect that coincided with the quake in both time and space, thus confounding our estimates: Neither Mao's death in 1976 nor the one-child policy enacted in the late 1970s Li et al. (2010) could have caused the variation we exploit for causal inference.

Fourth, we address the issue of migration: Since we are using a single cross-section in 2000, it is possible that the apparent long-run effect is spurious and based on migration flows. For example, it could be that heavily destroyed regions were more attractive following the reconstruction, fostering immigration. This, however is not likely the case: First, migration has been largely restricted due to the Chinese *hukou* system until the 1980s (Bao et al., 2009). Second, we provide evidence that migration would have, at best, downward biased our estimates: Using the census, we constructed $migration\_rate$, a dependent variable that captures the share people in each cohort who have not been born in the county of current residence. The main effect is highly significant and negative, suggesting that cohorts and counties affected by the earthquake had lower levels of migration (Table 6, Column I-III). This effect, however, is not economically large but rules out migration as a possible econometric concern.

Fifth, we test for the common trend assumption by comparing the pre-1976 trend of the outcome variables across counties. If the common trend assumption is violated, with treatment counties following a different trend than control countries, we may erroneously attribute the main effect to the treatment. In Table 7 we test for differential trends before the earthquake but cannot reject common trends except for the secondary completion sex ratio. The results, however, suggest that the trend for treated regions was lower: If so, the large effect - a doubling of the completion sex ratio - will even be a lower bound estimate, with the unbiased coefficient likely larger. Finally, Table 8 presents the simplified 2-period DiD we employ to ensure that our results are not driven by serial-correlation. While the remaining main effects remain robust, the effect of the earthquake on unemployment rate turns marginally significant. As we lose most of the controls using this method, this exercise is deemed to produce less precise estimates. The surprising robustness should hence be taken as additional evidence for the internal validity of the estimates.

4 Discussion

Even though the county-level relationship is robust and causal, our empirical strategy does not explicitly uncover the underlying transmission mechanisms. In order to embed our results in the related body of literature, we complement the findings by offering a few plausible channels through which individual decisions sum up to an aggregate effect:

First, the positive fertility response is likely to be a combination of replacement effects, shifting preferences for children and household formation: Based on the rich DHS data for three recent earthquakes, Finlay (2009) found that post-earthquake fertility responses were generally in excess of the sole response to child mortality. If the shock increased the baseline risk, even individuals suffering *no* losses could decide for more children to insure against a future shock. This is consistent with results in Poertner (2008), where an increase in the risk of hurricanes is associated with higher fertility for households with land. In addition, the shock could also induce a short-run shift in norms. Rodgers et al. (2005), for example, provide evidence for a fertility increase following a man-made (terrorist) shock, attributing the effect to "the feelings of immediate threat to life" and return to family values. Finally, post-quake re-marriages are likely to play a role, too. According to Yong (1988), about 15.000 households were affected by the death of either spouse. With family rebuilding peaking in the first half of 1977, it is likely that the new household formations were at least partly responsible for the fertility rise.

Second, there are two main channels through which these cohorts could exhibit adverse educational outcomes: On the one hand, the earthquake translated into a supply side shock, destroying schools and other infrastructure. Akbulut-Yuksel (2009), for example, identifies the destruction of schools as a main channel through which WWII bombing in Germany translated into adverse educational attainment for the affected cohorts in schooling age. On the other hand, Tangshan was not subject to a cumulative bombing shock but a one-off shock. Given that temporary classrooms were built by the end of the year (Yong, 1988) and since our treatment cohort was enrolled only years after the quake, there is likely to be another channel: Indeed, Akbulut-Yuksel (2009) also shows that the shock affected children exhibited lower health outcomes. If the adverse conditions after the earthquake translated into worse nutrition, the reduced education attainment could also be driven by adverse cognitive effects (Maccini and Yang, 2009).

Third, the substantial shift towards a male biased education enrollment ratio suggests that intrahousehold dynamics play a crucial role in Tangshan. Qian (2008), for example, provides evidence that an increase in female income improves survival rates for girls based on a natural experiment in Chinese tea regions. While our study has not found any evidence for differential neglect of girls or female infanticide based on the cohort sex ratio, the increased enrollment of boys suggests that the adverse shock resulted in a diversion of resources away from girls, in line with Hannum et al. (2007).

Finally, it is unclear why lower education attainment has not translated into adverse labour market outcomes, as proxied by higher unemployment rates. Indeed, it is possible that wage earnings are lower for the less educated while the unemployment rates remained the same - but given the lack of data, it is impossible to test this hypothesis. Alternatively, it could as well be that other factors - such as social networks or party affiliation - play a more improtant role in a transition country. Meng and Gregory (2002), for example, exploit the Cultural Revolution as a natural experiment to identify effects of lower schooling on earning outcomes. Surprisingly, the effect of interrupted education did not yield significant earning differences, consistent with our results.

4.1 Conclusion

Drawing upon county-level census data, this paper established a long-run relationship between a historical earthquake and current socio-economic outcomes: Our evidence suggests that the earthquake has not only induced a large fertility response, but also had adverse impacts on educational outcomes. While there is no evidence of a change in cohort sex-ratio, the results indicate that the earthquake has significantly skewed the sex composition in education. Despite the larger and less educated post-earthquake cohorts, there is no evidence for adverse labour market outcomes.

These findings are not only in line with the existing literature but also provide confirmatory evidence for hypotheses regarding the long-run effect of large environmental shocks. While the positive fertility response and adverse effect on education is a somewhat established stylized fact, we are (to the best of our knowledge) the first to reliably demonstrate its long-run effect: Even though Tangshan has been rebuilt, the generations born right after the earthquake still suffer from a quake that occured 25 years ago. While much of the debate and discourse on post-disaster relief is focused on the short-run response, interventions should also focus on cushioning long-run dynamics.

This paper has also shown that it is possible to obtain clean and causal results based on a single cross-section. Despite utmost effort in challenging our findings, the results remained remarkably robust, even improving in some cases. Nonetheless, this paper leaves many questions unanswered. Confined only to the county-level, it has not been possible to offer rigorous evidence for plausible channels through which these effects are transmitted. By drawing upon related studies, we hoped to offer a few hypothesized mechanisms. Given the lack of data, however, our results are as closest as one can get.

Finally, the paper has only focused on a narrow set of outcome variables. The literature on complex emergencies, however, shows that the impacts of disasters are much broader (Keen, 2007). Even today, there is still anecdotal evidence of long-term psychological effects among the survivors of the Tangshan earthquake (Feng, 1992). While exploring all these dimensions would lie beyond the scope of this paper, future work can focus on other indicators of well-being.

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5 Appendix

Figure 1: Tangshan shortly after the earthquake (China Earthquake Administration)

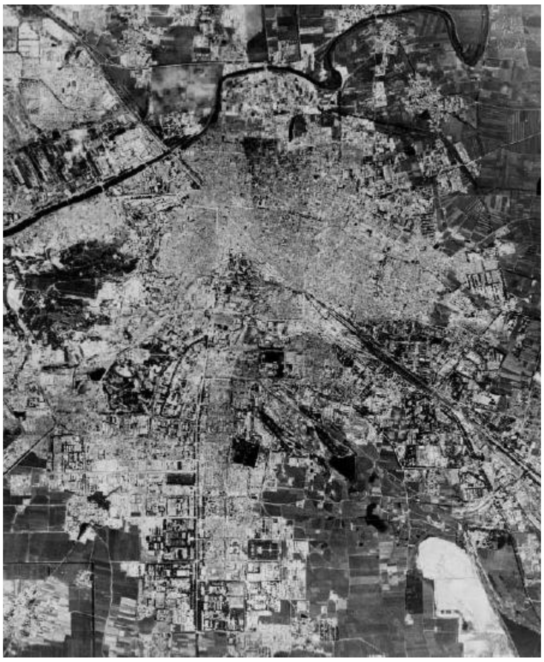

Figure 2: Aerial view of damage in downtown Tangshan (China Earthquake Administration)

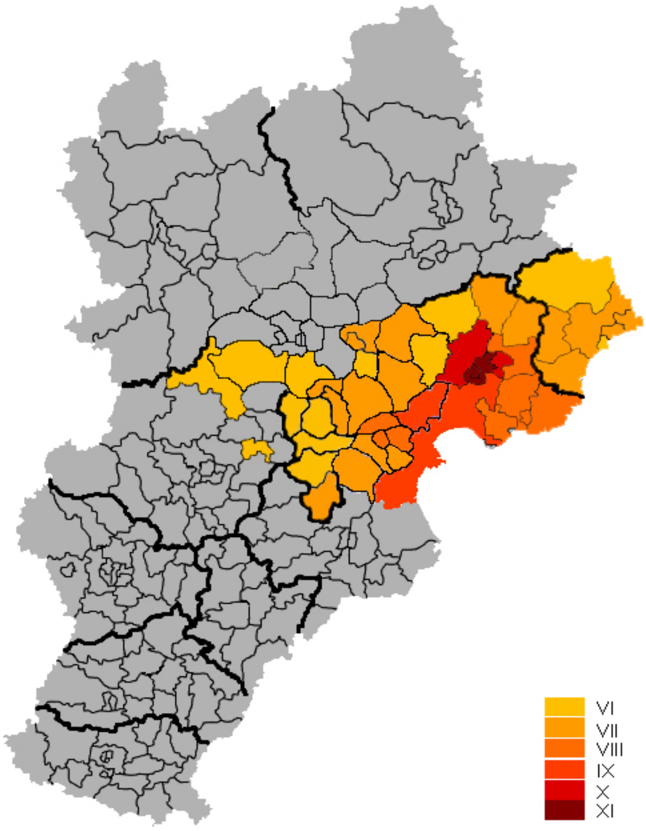

Figure 3: County-level differential earthquake intensity

	Total				Urban				Rural			
	Pooled	Treatment	Control	Diff	Pooled	Treatment	Control	Diff	Pooled	Treatment	Control	Diff
Cohort size	7401.39	6546.80	7488.30	-941.50+	6660.76	7285.04	6564.72	720.32	7623.58	6177.68	7749.31	-1571.63+
	(128.26)	(144.14)	(135.39)	(408.15)	(271.69)	(514.63)	(303.07)	(597.25)	(144.25)	(511.84)	(149.19)	(533.14)
N	715	66	649		165	22	143		550	44	506	
Cohort sex ratio	1.037	1.012	1.040	-0.027+	1.041	0.986	1.049	-0.063+	1.037	1.026	1.038	0.011
	(0.009)	(0.008)	(0.002)	(0.009)	(0.005)	(0.010)	(0.005)	(0.011)	(0.003)	(0.011)	(0.003)	(0.011)
N	715	66	649		165	22	143		550	44	506	
County area	1004.78	405.6	1410.39	-1004.78+	146.99	89	155.91	66.91+	1713.14	616.66	1789.64	-1172.97+
	(96.96)	(64.03)	(72.81)	(96.96)	(12.20)	(5.23)	(13.91)	(14.86)	(83.05)	(89.66)	(87.55)	(125.32)
N	671	55	616		165	22	143		550	33	473	
Secondary completion rate	0.411	0.504	0.402	0.102+	0.458	0.533	0.446	0.087+	0.397	0.489	0.389	0.099+
	(0.004)	(0.005)	(0.005)	(0.005)	(0.009)	(0.007)	(0.010)	(0.012)	(0.005)	(0.014)	(0.005)	(0.015)
N	715	66	649		165	22	143		550	44	506	
Sec. enroll. sex ratio	1.564	1.289	1.592	-0.30+	1.149	1.177	1.145	-0.032	1.688	1.344	1.718	-0.373+
	(0.019)	(0.027)	(0.021)	(0.034)	(0.012)	(0.021)	(0.014)	(0.026)	(0.022)	(0.036)	(0.023)	(0.043)
N	715	66	649		165	22	143		550	44	506	
Unemployment rate	0.013	0.015	0.013	0.001*	0.018	0.014	0.018	-0.003+	0.012	0.015	0.012	-0.0027+
	(0.0002)	(0.0006)	(0.0002)	(0.0006)	(0.0003)	(0.0005)	(0.0004)	(0.0007)	(0.0003)	(0.0009)	(0.0003)	(0.0009)
N	715	66	649		165	22	143		550	44	506	
Unemployment sex ratio	0.258	0.290	0.255	0.034**	0.418	0.384	0.424	0.040	0.210	0.242	0.207	0.035**
	(0.006)	(0.015)	(0.007)	(0.016)	(0.017)	(0.022)	(0.019)	(0.029)	(0.005)	(0.016)	(0.005)	(0.017)
N	714	66	648		164	22	142		550	44	506	

Table 1: Pre-shock summary statistics by treatment status

	Dependent variable: *log_cohort_size*					
	(I)	(II)	(III)	(IV)	(V)	(VI)
intensity	0.018	0.010	0.106	0.106*	0.107*	0.110*
	(0.05)	(0.05)	(0.06)	(0.06)	(0.06)	(0.06)
quake_lag	-0.161+	-0.585+	-0.651+	-0.577+	-0.401+	-0.712+
	(0.02)	(0.09)	(0.07)	(0.08)	(0.08)	(0.10)
$intensity \times quake$		0.058+	0.068+	0.069+	0.068+	0.063+
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
log_area			0.139	0.151	0.183*	0.219**
			(0.10)	(0.10)	(0.09)	(0.09)
urban			0.160	0.179	0.231	0.289
			(0.33)	(0.32)	(0.30)	(0.29)
prov_cbr				-0.007	-0.045+	-0.119+
				(0.01)	(0.01)	(0.02)
prov_cdr				-0.071+	-0.110+	0.092+
				(0.00)	(0.01)	(0.02)
year_trend					-0.164+	-0.533+
					(0.02)	(0.07)
$year\_trend^2$					0.004+	0.013+
					(0.00)	(0.00)
Year fixed effect	No	No	No	No	No	Yes
Observations	1365	1365	1281	1281	1281	1281
Number of counties	65	65	61	61	61	61
R^2	0.010	0.012	0.087	0.111	0.193	0.312

Table 2: Earthquake intensity on (log) cohort sizes, county-level

Notes: Estimated by OLS. Numbers in parentheses are robust SEs clustered at the county level. Intercept not reported. * $p < 0.1$, ** $p < 0.5$, + $p < 0.01$.

Dependent variable: various outcome indicators					
	(I)	(II)	(III)	(IV)	(V)
intensity	-0.002	0.003	-0.040**	-0.000	0.001
	(0.01)	(0.01)	(0.02)	(0.00)	(0.01)
quake_lag	-0.108*	0.301+	-0.973+	-0.009*	-0.196
	(0.05)	(0.09)	(0.29)	(0.00)	(0.13)
intensity × *quake*	0.006	-0.023+	0.087+	0.000	0.033*
	(0.01)	(0.01)	(0.02)	(0.00)	(0.02)
log_area	-0.011	-0.036	0.095+	-0.002*	-0.048+
	(0.01)	(0.03)	(0.03)	(0.00)	(0.01)
urban	-0.024	-0.141*	-0.017	0.007**	0.037
	(0.03)	(0.08)	(0.09)	(0.00)	(0.04)
prov_cbr	-0.013+	0.008	-0.002	-0.001*	-0.016**
	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)
prov_cdr	0.024+	-0.028*	0.074*	0.000	0.004
	(0.01)	(0.02)	(0.04)	(0.00)	(0.01)
year_trend	-0.041+	0.003	-0.072**	-0.025+	-0.279+
	(0.01)	(0.02)	(0.03)	(0.00)	(0.02)
$year\_trend^2$	0.001+	-0.000	0.002+	0.000+	0.005+
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Year fixed effect	Yes	Yes	Yes	Yes	Yes
Number of counties	61	61	61	61	61
Observations	1281	1281	1281	1220	1219
R^2	0.135	0.325	0.427	0.879	0.777

Table 3: Earthquake intensity on secondary outcome variables

Notes: Estimated by OLS. Numbers in parentheses are robust SEs clustered at the county level. Intercept not reported. Column I: Cohort sex ratio. Column II: Cohort sec. completion rate. Column III: Cohort sec. completion sex ratio. Column IV: Cohort unemployment rate. Column V: Cohort unemployment sex ratio. * $p < 0.1$, ** $p < 0.5$, + $p < 0.01$

	Dependent variable: *log_cohort_size*					
	(I)	(II)	(III)	(IV)	(V)	(VI)
log_epicentre	-0.062	-0.049	-0.084	-0.092	-0.115*	-0.142**
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
quake_lag	-0.160+	0.237+	0.233+	0.327+	0.540+	0.189**
	(0.02)	(0.05)	(0.05)	(0.05)	(0.06)	(0.07)
$log\_epicentre \times quake$		-0.089+	-0.090+	-0.094+	-0.101+	-0.102+
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
log_area			0.098	0.112	0.150*	0.192**
			(0.09)	(0.09)	(0.09)	(0.08)
urban			0.105	0.128	0.187	0.255
			(0.34)	(0.33)	(0.31)	(0.29)
prov_cbr				-0.009	-0.049+	-0.129+
				(0.01)	(0.01)	(0.02)
prov_cdr				-0.071+	-0.114+	0.096+
				(0.01)	(0.01)	(0.02)
year_trend						-0.563+
						(0.07)
$year\_trend^2$						0.013+
						(0.00)
Year fixed effect	No	No	No	No	No	Yes
Observations	1365	1365	1281	1281	1281	1281
Number of counties	65	65	61	61	61	61
R^2	0.016	0.018	0.05	0.082	0.174	0.307

Table 4: Robustness check: Distance from epicentre on (log) cohort sizes, county-level

Notes: Estimated by OLS. Numbers in parentheses are robust SEs clustered at the county level. Intercept not reported. * $p < 0.1$, ** $p < 0.5$, + $p < 0.01$.

Dependent variable: *log_cohort_size*				
	(I)	(II)	(III)	(IV)
intensity	0.110*	0.108*	0.118**	0.121**
	(0.06)	(0.06)	(0.06)	(0.06)
quake_lag	-0.712+	-0.733+		
	(0.10)	(0.10)		
intensity × *quake*	0.063+	0.068+		
	(0.01)	(0.01)		
*quake_lag*73			0.618+	
			(0.12)	
intensity × *quake*73			0.007	
			(0.01)	
*quake_lag*70				1.158+
				(0.18)
intensity × *quake*70				-0.013
				(0.01)
log_area	0.219**	0.191**	0.219**	0.219**
	(0.09)	(0.09)	(0.09)	(0.09)
urban	0.289	0.244	0.289	0.289
	(0.29)	(0.30)	(0.29)	(0.29)
prov_cbr	-0.119+	-0.054+	-0.119+	-0.119+
	(0.02)	(0.01)	(0.02)	(0.02)
prov_cdr	0.092+	-0.123+	0.095+	0.095+
	(0.02)	(0.01)	(0.02)	(0.02)
year_trend	-0.533+	-0.040	-0.666+	-0.667+
	(0.07)	(0.03)	(0.08)	(0.08)
year_trend^2	0.013+	0.003+	0.015+	0.015+
	(0.00)	(0.00)	(0.00)	(0.00)
before		-0.599+		
		(0.10)		
Year fixed effect	Yes	Yes	Yes	Yes
Observations	1281	1281	1281	1281
Number of counties	65	65	61	61
R^2	0.312	0.217	0.309	0.309

Table 5: Robustness check: Placebo test using different timing dummies

Notes: Estimated by OLS. Numbers in parentheses are robust SEs clustered at the county level. Intercept not reported. * $p < 0.1$, ** $p < 0.5$, + $p < 0.01$.

Dependent variables:	*log_migration_rate*			*log_migration_sex*		
	(I)	(II)	(III)	(IV)	(V)	(VI)
intensity	0.003+	0.001	0.001	-0.005	-0.033*	-0.033
	(0.00)	(0.00)	(0.00)	(-0.02)	(0.02)	(0.02)
quake_lag	0.012+	0.012+	-0.002	-0.513+	-0.545+	-0.179
	(0.00)	(0.00)	(0.00)	(0.14)	(0.16)	(0.176)
intensity × *quake*	-0.001**	-0.001**	-0.001**	0.040**	0.044**	0.04*
	(-0.00)	(0.00)	(0.00)	(0.02)	(0.02)	(0.02)
log_area		-0.002**	-0.001		-0.081**	-0.072*
		(0.00)	(0.00)		(0.03)	(0.04)
urban		0.013+	0.015+		-0.085	-0.071
		(0.00)	(0.00)		(-0.10)	(0.11)
prov_cbr			-0.002+			-0.011
			(0.00)			(0.01)
prov_cdr			0.002+			-0.016
			(0.00)			(0.07)
year_trend			0.004+			-0.302+
			(0.00)			(0.04)
$year\_trend^2$			-0.000**			0.006+
			(0.00)			(0.001)
Year fixed effect	No	No	Yes	No	No	Yes
Observations	1365	1281	1281	1363	1279	1279
Number of counties	65	65	61	65	65	61
R^2	0.092	0.419	0.682	0.03	0.063	0.243

Table 6: Robustness check: Effect of earthquake on migration

Notes: Estimated by OLS. Numbers in parentheses are robust SEs clustered at the county level. Intercept not reported. * $p < 0.1$, ** $p < 0.5$, + $p < 0.01$.

	Dependent variable: various outcome indicators					
	(I)	(II)	(III)	(IV)	(V)	(VI)
treat	-0.195	-0.015	0.079	0.499*	0.010	-0.047
	(0.58)	(0.08)	(0.10)	(0.29)	(0.01)	(0.19)
year	0.040+	0.001	-0.007+	0.053+	-0.000+	-0.004*
	(0.01)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)
treat × *year*	0.003	-0.000	0.001	-0.028+	-0.000	0.003
	(0.01)	(0.001)	(0.01)	(0.01)	(0.00)	(0.01)
Number of counties	61	61	61	61	61	61
Observations	1365	122	122	122	122	122
R^2	0.007	0.007	0.190	0.259	0.346	0.67

Table 7: Robustness check: Testing the common trend assumption

Notes: Estimated by OLS. Numbers in parentheses are robust SEs clustered at the county level. Intercept not reported. Column I: Cohort size. Column II: Cohort sex ratio. Column III: Cohort sec. completion rate. Column IV: Cohort sec. completion sex ratio. Column V: Cohort unemployment rate. Column VI: Cohort unemployment sex ratio. * $p < 0.1$, ** $p < 0.5$, + $p < 0.01$

Dependent variable: various outcome indicators						
	(I)	(II)	(III)	(IV)	(V)	(VI)
$intensity$	0.063	0.004	0.011	-0.070+	0.000	0.042+
	(0.05)	(0.01)	(0.01)	(0.02)	(0.00)	(0.01)
$before$	-0.323+	-0.029	0.308+	-0.632+	0.000	0.515+
	(0.12)	(0.04)	(0.04)	(0.14)	(0.01)	(0.09)
$intensity \times quake$	0.055+	0.007	-0.027+	0.057+	0.002**	-0.006
	(0.01)	(0.01)	(0.01)	(0.02)	(0.009)	(0.01)
Number of counties	61	61	61	61	61	61
Observations	122	122	122	122	122	122
R^2	0.047	0.039	0.190	0.259	0.346	0.67

Table 8: Robustness check: Simple 2-period DiD

Notes: Estimated by OLS. Numbers in parentheses are robust SEs. Intercept not reported. Column I: Cohort size. Column II: Cohort sex ratio. Column III: Cohort sec. completion rate. Column IV: Cohort sec. completion sex ratio. Column V: Cohort unemployment rate. Column VI: Cohort unemployment sex ratio. * $p < 0.1$, ** $p < 0.5$, + $p < 0.01$